OUR BOOK SHELF

Elementi di Fisica. Vol. IV., Elettricità e Magnetismo. By Prof. Antonio Ròiti. (Florence, 1883.)

SURELY, and not slowly, the views of Thomson, Maxwell, and the modern electricians generally are finding acceptance throughout the Continent. The absolutely unanimous acceptance of the British Association's system of electrical units since the indorsement of that system by the Paris Congress of 1881 has proved the immense gain to the electrical world of having a uniform means of expressing electrical quantities, and has compelled electricians not only to read but to comprehend the writings of the pioneers of this most important reform. The work now before us for review, though professing to be but a text-book for use in the lyceums and schools of Italy, gives ample evidence that its author, Prof. Ròiti, of the Royal Institute of Higher Studies in Florence, is not only abreast of all the latest developments of electricity, but that he has mastered the theory also. Few text-books of its size have we seen that will compare favourably with Prof. Ròiti's little volume of 356 duodecimo pages. The faults which have been hitherto so conspicuous in most of the Continental text-books on electricity are in this work conspicuously absent. As an example we may refer to the author's treatment of the relation between the capacities, potentials, and charges of similar conductors. The elementary theory of the magnetic shell and that of the mutual potential of two magnetic shells are neatly expounded in pages 131 to 133. The absolute electrometer and the quadrant electrometer of Sir W. Thomson are both described, and illustrative figures given. The system of absolute and derived (C.G.S.) units, and that of the practical units of electric quantities based upon them, are explained at length on pages 204-5. There is a short chapter on the electric light, and another on electric motors, in which the *anello elettromagnetico di Pacinotti* is described, the author remarking with emphasis that it contained the germ of almost all the machines by which the marvellous strides recently made in the applications of electricity have been achieved. The experiments of Deprez at Paris on the electric transmission of power, and the economic questions involved are also touched upon. Crookes's researches on "radiant matter" are mentioned and illustrated. Amongst points of novelty may be mentioned Pellat's method of measuring the electromotive force due to polarisation, which has not yet, we believe, found its way into any English text-book. Two points of criticism we have to offer in conclusion. The first is that the author defines electric *tension* as identical with the electric *force*, equal to 4π times the surface density of the charge, instead of defining it, in the sense of Faraday and Maxwell, as the stress on the dielectric, which is proportional to the square of the surface density, and therefore proportional also to the square of the electric force or electromotive intensity at the point of the surface considered. The only other complaint we have to make of the work—and this does not detract greatly from its value—is that the author does not acknowledge the sources from which some of his descriptions and cuts are taken. S. P. T.

Dr. H. G. Bronn's Klassen und Ordnungen des Thier-Reichs, wissenschaftlich dargestellt in Wort und Bild. Erster Band, Protozoa. Neu bearbeitet von Dr. O. Bütschli. (Leipzig and Heidelberg: C. F. Winter, 1880-83.)

THE first nineteen parts of this new edition of vol i. of Dr. Bronn's well known and important work on the classes and orders of animals, nearly completing the volume, prove that Prof. Bütschli has spared no pains to keep it up to the most modern investigations of the Protozoa. In no one division of the animal kingdom has observation gone so hand in hand with discovery as in this, the lowest of her classes. Glancing at the portion treating of the Gregarinida, what strides have been made in our knowledge of these forms within the last ten years. Adopting Leuckart's titles for the class of Sporozoa, under which are the sub-classes Gregarinida, Coccidia, Myxosporidia, and Sarcosporidia, we find 137 pages and eight plates crowded with figures devoted to a sketch of the characteristics of the class with diagnoses of the genera and the number of species, and references to the places where fuller details of these latter will be found. The illustrations are clear and effective, and copied from every available source. The bibliography appears to be well to date, and this volume when complete will be an indispensable handbook for the student of the lower forms of animal life.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

"Elevation and Subsidence"

THE view that the glacial subsidence was due to the pressure of the accumulating land ice, has been accompanied with the corollary that subsequent elevation was due to the removal of this pressure by the melting of the ice; but though I think the first is true, the corollary is not so, in England at least.

In my memoir "On the Newer Pliocene Period in England" (*Quart. Journ. Geol. Soc.* for 1880, p. 457, and 1882, p. 667), I have endeavoured to show how the inclination of this country changed during the progress of the major glaciation, and the flow of the land ice from the mountain districts to the sea altered in accordance therewith, as well as pointed out (p. 709) the connection of this change of inclination with the accumulation of the land ice on the mountain districts; but I have also traced in detail in it how the east side of England rose to an extent that brought Norfolk and Suffolk from a submergence of more than 300 feet to their present level at least, and Essex proportionately so, while the land ice continued to push over the sea-bed of sand and gravel, as this rose into land, covering it with its moraine, until by this rise the easterly movement of the ice was arrested, while the west and south of England still remained to a great extent submerged. Since that memoir was published, Mr. David has in the same journal described the glacial clay which represents the moraine of the Welsh land ice in East Glamorganshire, itself *un*covered by any marine deposit, as covering beds of stratified sand and gravel, which, from their containing many chalk flints, can be only the bottom of the antecedent sea, as low down as 80 feet above Ordnance datum. When this is compared with the evidence of more than 1300 feet of submergence afforded by the shell bearing gravels of North Wales; of 700 feet afforded by the Gloucestershire gravels to the east; and of between 500 and 600 feet afforded by the gravels of Devon to the south of Glamorganshire, it becomes evident that the amount of rise which took place in the west of England before the land ice began to retreat was even greater than in East Anglia. It is to subterranean movements engendered by this pressure, and not to its removal, that the rise in England seems to me to have been due; and I have given several sections in this memoir in illustration of the abrupt and violent character of the upthrows connected with it.

Although in this memoir I remarked upon the coincidence of the westerly increment in the great submergence with the augmenting quantity of the land ice on Cumberland, Westmoreland, and Wales, as the major glaciation went on, yet this coincidence between augmenting land ice and submergence is, I now see, more complete than had then occurred to me; for though I described the evidences that show the passage from the Crag to the glacial marine beds of Norfolk and Suffolk to have been accompanied by a northerly subsidence which submerged the valley of the Crag river, in the north of the former county, while the other extremity of its estuary (in East Suffolk) was elevated, so that islands formed of Crag beds came there into

existence, around and up to which the earliest glacial marine accumulations of sand and shingle were bedded, and which, as subsequent southerly and westerly subsidence ingulphed all but the highest downs of the south of England, eventually spread over these islands, yet I did not connect this first movement with the pressure of the land ice. I have since, however, perceived that this connection exists; for, as the Glacial period came on, the precipitation must necessarily, on account of latitude, have taken more exclusively the form of snow in Scotland before it did so in Cumberland and Wales; and, by thus accumulating land ice earlier in Scotland, caused this northerly subsidence. As the cold increased the precipitation in the form of snow reached its maximum in Westmoreland and Cumberland, and yet later somewhat in Wales; and as it did so, the pressure of the land ice engendered by it turned the depression increasingly in those directions, so that eventually all England, save the highest downs, and even the lower ends of the river valleys of North-Western France became submerged proportionately to their contiguity to the foci of pressure. These increments of depression I have in this memoir traced by more than one train of evidence, and shown how this change of inclination, by diverting the directions taken by the land ice to the sea, changed also the character of the materials of which the resulting morainic clay is made up, and so gave rise to those Upper and Lower clays of the major glaciation in Yorkshire, which have been seized upon to support the hypothesis of alternations of climate during that glaciation.

The connection between the augmenting weight of the land ice and subsidence seems to me so clear, that I cannot but think that American geologists have fallen into an error, in regarding the Champlain period as belonging to the wane of the great glaciation, instead of to its culmination. It seems to me that although the increasing volume of the land ice in the Lake (or St. Lawrence) basin caused this ice at its western extremity, where the parting between the two basins is very low, to invade the upper part of the great Mississippi basin, yet its weight where thickest—that is to say, towards its eastern extremity, which was that of greatest snow precipitation—so pressed this extremity down that the seaward termination of this ice in the Gulf of St. Lawrence retreated before the greater depth of sea there which thus resulted, and so allowed the sea to penetrate to Montreal and Lake Champlain, near the former of which places the remains of its inhabitants have been left at an elevation of about 600 feet.

With all this, however, we must not be led into regarding all movements of subsidence as a result of increasing accumulations, whether of sediment or otherwise; for such is evidently not the case, though to instance this would lead me beyond the object of this letter. SEARLES V. WOOD

Martlesham, near Woodbridge, October 11

THE above remarks require but little comment, and chiefly tend to show that Mr. S. V. Wood attaches increased importance to the idea that weight produces subsidence. He speaks of elevation commencing before the retreat of the glaciers, but that they would be enormously lightened before retreating is a fact that I can hardly suppose he has overlooked. In ascending the Jungfrau many years ago, when the Swiss glaciers were diminishing, I crossed from the Grindelwald on to the Aletsch, and had to descend a cliff of nearly vertical ice, which my recollection tells me was some sixty feet high, in order to pass from one to the other. The difference in level was caused by the extra rapid melting of the Aletsch, owing to its more southern aspect and exposure to the Föhn wind. This was at the head of the glacier, and the melting was much more rapid lower down, though the superficial area had not contracted to any appreciable extent. This loss of weight would lead to elevation long before the disappearance of the ice. J. STARKIE GARDNER

Snake Bite

I WAS an eye-witness to the following:—My brother was walking within a field of the Land's End when he stooped to pick up a large snake, apparently nearly a yard long, which bit him on the thumb. The bite became very painful in a few moments, and we realised for the first time that it was poisoned. In less than five minutes he was in the hotel and swallowed half a pint of neat brandy, and soon after some ammonia and water, without any effect. The wound had been well sucked and was steeped in ammonia, but the arm soon swelled to the size of the body, and the swelling began to extend down the ribs. The thumb was lanced while immersed in hot water, and the result was similar to the first gashes in a shoulder of mutton, the exposed flesh being dark mulberry colour, and not a drop of blood flowing. He recovered in seven or eight days, but was weak for some time. J. S. GARDNER

Park House, St. John's Wood Park, N.W.

The Observation of Meteors

ACCOUNTS of large meteors form a frequent subject of correspondence in the columns of scientific journals, but it is not often the case that the descriptions of these phenomena are sufficiently exact to be valuable for purposes of calculation. Rough estimates of the direction and position of flight are of little utility, and the vague statements often made occasion an endless source of difficulty in the satisfactory reduction of results. It is true that observers of fireballs are generally taken unawares by the suddenness of the apparitions, and that the visible paths are seldom to be noted accurately. Before the observer collects himself to record the facts of the display it has disappeared, and he has to rely solely upon the impressions retained in his memory.

But, notwithstanding this drawback, the observations of large meteors as published from time to time would possess far greater scientific value if observers would attend more scrupulously to that most essential detail, the *direction of flight*, and express it by some method of uniformity. Sometimes we find the path vaguely stated as being from "east to south," without any attempt to estimate the altitude of the beginning and end points of the course. On other occasions a meteor is described as passing above or below certain stars or planets. The latter method, though an improvement upon the former, is to some extent indefinite, and therefore unsatisfactory, as giving unnecessary trouble to those who undertake the reduction of such materials. For instance, a meteor is observed early in August, 1881, shooting from "some distance below Saturn towards Comet B." Now in reducing this account troublesome references have to be made to find the places of the two objects on the dates mentioned, and then we are left to guess at the "distance below Saturn" implied in the description. These objections would disappear, and the comparison of observations be greatly facilitated, could observers be induced to give the right ascension and declination of the beginning and end points of the visible paths. These elements admit of ready determination by projecting the observed flights upon a star chart or celestial globe and reading them off. Even in cases where the observations are uncertain, the observer should fix the path according to this method as nearly as possible, for it is manifest that it is infinitely preferable to the vague and often worthless attempts to guess altitudes, compass bearings, &c., and, moreover, it renders the after comparison of observations a work of greater facility and precision.

Though the direction of flight is the all-important element to be determined by meteor observers, there are some minor points which should also be carefully recorded. The time of appearance, brightness, approximate duration, and whether accompanied by phosphoric streaks or spark trains, are each important in their way, and must be stated whenever feasible. If this were done more systematically, the observations of fireballs would acquire additional value, and may quite possibly develop some new facts either as to their appearance or origin.

Bristol, October 22 W. F. DENNING

"Partials"

IT is a well known fact that no musical sound is produced alone, but the instant it is sounded a series of other sounds springs from it, and always in a certain order and ratio. Next to the primary tone, the octave is heard, then the octave fifth, the double octave, the double octave third, the double octave fifth, the extra flat double octave seventh, the treble octave, and so on. The origin of these "partials" has long been an interesting study, and a solution has occurred to me which I think is the true one.

We have the fact that an object seen by the eye for ever so short a time leaves its impression on the optic nerves about the eighth of a second *after it has passed away*. By analogy it seems highly probable that all our nerves, including those of the